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The Origin of Species: Evolutionary Taxonomy as an Example of the Rhetoric of Science

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The rhetoric of science discipline was born late because a persistent dream of the West died hard: the dream of certain truth concerning an independent reality. Assuming its availability, philosophers from Plato to Carnap could divide science from rhetoric; suspecting its likelihood, we cannot. For us, therefore, the rhetoric of science is a branch of inquiry having as its goal "to find out in each case the existing means of persuasion" (Aristotle 1355^{b}14). Accordingly, the rhetoric of science does not look at the texts of natural science as transparent vehicles by means of which knowledge is conveyed: it looks through these texts, to persuasive structures. Since the establishment of knowledge claims in natural science is paramount, the rhetorical analysis of natural science has as its primary task the rhetorical reconstruction of the means by which scientists convince themselves and others that the assertions they make are an integral part of the privileged activity with which they are allied.

The rhetoric of science analyzes scientific texts by the same means as others analyze speeches and literature; it does not recognize that the sciences differ from the humanities in any fundamental way: it denies especially that the sciences differ in that "they do not have first to gain access to their object domain through hermeneutic means" (Habermas 1982, 274). As Latour and Woolgar (1979, 261) make explicit, "the basic prototype of scientific activity is not to be found in the realm of mathematics and logic but . . . in the work of exegesis. Exegesis and hermeneutics are the tools around which the idea of scientific production has historically been forged." Holding these views, we are obliged to see the enterprise of

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science hermeneutically, as a stream of texts exhibiting generally an epistemology based on prediction and control, but available nonetheless to an epistemology based on understanding. By recourse to an insight as old at least as Vico, we can view the texts of natural science as products of human interaction: phenomena to be understood in the way human beings understand each other.

From a rhetoric of science so conceived, no feature is exempt from rhetorical explanation. As we shall see, even prediction itself, as a category of judgment, is not exempt. In principle, then, the rhetoric of science discipline is as complete as the history, philosophy, or sociology of science. And as incomplete. For Gadamer's point about physics applies to any discipline that insists on ultimate coherence:

> The world of physics cannot seek to be the whole of what exists. For even a world formula that contained everything, so that the observer of the system would also be included in the latter's equations, would still assume the existence of a physicist who, as the calculator, would not be an object calculated (Gadamer 1975, 410).

The history, philosophy, sociology, and rhetoric of science—all must be faithful to their texts. But the rhetoric of science differs from its sibling disciplines: it must, to paraphrase Barthes, "star" its texts (1974, 13); it must use them in a special way. Only by looking at them as linguistic entities can it look through them *sub specie rhetoricae,* to their underlying persuasive structures.

RECONSTRUCTING EVOLUTIONARY TAXONOMY

A complete rhetoric of science must be able to reconstruct the natural sciences without remainder. In this paper, I want to test the hypothesis of completeness against evolutionary taxonomy, the science of classifying animals and plants as species in accordance with evolutionary theory. If a rhetoric of this science is possible, we must be able to reconstruct its central concept, the species, rhetorically without remainder. But our route to this goal must be indirect. We cannot translate the concept of species directly into rhetorical terms because we must avoid the claim that what is being translated is precisely what is *not* scientific, that after our rhetorical analysis there still remains an essential core of untranslatable scientific meaning.

To avoid this claim, we must reconstruct the species of evolutionary taxonomy rationally in the hope that the result will be recognizably a version both of this particular scientific concept and of "the methodological ideal . . . that dominates modern mathematically based natural science" (Gadamer 1975, 414); we must prepare a "schematized description of an imaginary procedure, consisting of rationally prescribed steps, which would lead to essentially the same results as the actual [historical or] psychological process" (Carnap 1963, 16). In a rational reconstruction of the species of evolutionary taxonomy, when stages are referred to—stages at which species are identified, defined, or redefined—these are not historical or psychological events, but analytical categories. It is this rational reconstruction of the species that we will translate without remainder into rhetorical terms.

I do not deny that infinitely many rational reconstructions are possible; that evolutionary taxonomists may argue with the details of my rational reconstruction; that they may even take the position that mine is the wrong rational reconstruction. But I would argue that these scientists cannot take the position that rational reconstruction is per se the wrong approach for extracting method from practice without denying that evolutionary taxonomy is a science. And that is precisely my point. To evolutionary taxonomists, ours must be a world inhabited by evolutionary species. To each of these can be applied, and from each of these can be inferred, at least in principle, a central process: the operation of natural selection on random genetic variation. Moreover, this reciprocal relationship between observation and theory presupposes objectivity: evolutionary taxonomy must describe a world whose existence is in some sense independent of its descriptions.

Our rhetorical reconstruction, on the other hand, redescribes evolutionary species by translating into rhetoric each aspect of our rational reconstruction: no aspect we have described as science remains without its rhetorical counterpart. By means of rhetorical reconstruction, evolutionary taxonomy is transformed into an interlocking set of persuasive structures. *Sub specie rhetoricae,* we do not discover, we create: plants and animals are brought to life, raised to membership in a taxonomic group, and made to illustrate and generate evolutionary theory. If a rhetorical reconstruction describes rhetorically every aspect that a rational reconstruction describes rationally, a complete rhetoric of science becomes possible.

THE RATIONAL RECONSTRUCTION OF SPECIES

In contemporary biology, species cannot be defined classically, by genus and differentia; the effort, in Wittgenstein's pungent phraseology, resembles "[trying] to find the real artichoke by stripping it of its leaves." Instead, a species must be defined by depicting and describing "family resemblances" among organisms (Wittgenstein 1965, 125).[1] Earlier, in *Origin of Species*, Darwin had placed this notion in a taxonomical context: "There are crustaceans at the opposite ends of the series, which have hardly a character in common; yet the species at both ends, from being plainly allied to others, and these to others, and so onwards, can be recognized as unequivocally belonging to this, and to no other class of the Articulata" (Darwin 1964, 419).

Identification: Family Resembances

When we define general terms like species according to family resemblances, we give up as spurious the precision of classical definition as it applies to the natural world. Nevertheless definition by family resemblance is real: it marks off a nonarbitrary class. Because I lack rules that will demarcate absolutely what I name as a new species of hummingbird, I can be puzzled about whether a particular organism belongs to the species, but I can't just drop anything in there, eagles for example. In addition, a good general term is an open class in two senses: there is always the possibility that I'll see a member I overlooked, and, anyway, at least for creatures not extinct, there is always the chance of genuinely new members. Finally, the application of a general term can be learned; that is, a learner can apply the term independently of the guidance of her teacher (Bambrough 1966).

Identification: Potential Species

In a typical paper in evolutionary taxonomy, "A New Species of Hummingbird from Peru" (Fitzpatrick, Willard, and Terborgh 1979), there is everywhere evident the extensive description and depiction that are the evidential grounding of family resemblance. These scientists, for example, record numerous qualitative visual impressions. Some are species-general, others sexually specific:

> A broad, pale buffy breast band separates the smaller throat spots from larger and more numerous discs on the breast and flanks. In a few specimens the posterior border of the breast band is entirely defined by a broad row of these discs. The belly is free of dark spots in all specimens. The downy crissum [anal region] is white as in males, and the undertail coverts [feathers at base] are dusky, edged Cinnamon. (p. 178)

In addition to qualitative visual impressions, Fitzpatrick and his co-workers define the potential species by recording its behavior. For example, we are afforded a detailed description of "male-female display":

> Initially a pair [of birds] was foraging around the walls of [a vine-covered sinkhole] and in the surrounding shrubs along the rim. Both male and female were observed perching on a rootlet, making frequent sallies to capture tiny flying insects. (p. 183)

To this abundance of qualitative information, quantitative data is added. We are given by degree, minute, and second the geographical coordinates of the area explored; to the nearest ten meters the height of the habitat above sea level; to the tenth of a millimeter the mean, range, and standard deviation of six specimen dimensions; to two decimal places the ratio of exposed culmen [upper ridge of a bird's bill] to wing chord length; to the second the length of calls.

But the most impressive attempt to establish the species is in the frontispiece, a full-color painting of two of the birds in situ: a dimorphic pair, the female perched at an angle which most clearly displays her distinctive tail and underparts. The male displays, not only his characteristic deeply cleft tail and dark coloring, but his wings extended and in rapid movement as he hovers. Moreover, the bird's feeding behavior is shown: the "deep purple petals [of its favorite flower] form a tubular corolla that hangs vertically . . . forcing the foraging [bird] to hover directly below and point its bill straight upward to retrieve the nectar" (p. 182).

The potential species is not only described and depicted in terms of family resemblances; by comparison and contrast, it is carefully differentiated from closely allied species. In this process of establishing species status, the character is the minimum unit of observation. Characters are parts of the organism or aspects of its behavior that most perspicuously establish it as a member of a genus or differentiate it as a separate species (Mayr 1982, 19–32). The harder a character works at either of these tasks, the more it is valued, the more heavily it is weighted.[2] In establishing their potential species as a member of a particular genus, Fitzpatrick and his co-workers emphasize "the relative bill length, nostril feathering, and well delineated nasal operculum [lid-like covering]" (Fitzpatrick, Willard, and Terborgh 1979, 181); in establishing the difference between the potential species and its fellows of the same genus, they give weight to the female's "elongated and deeply forked, entirely iridescent, metallic blue tail,

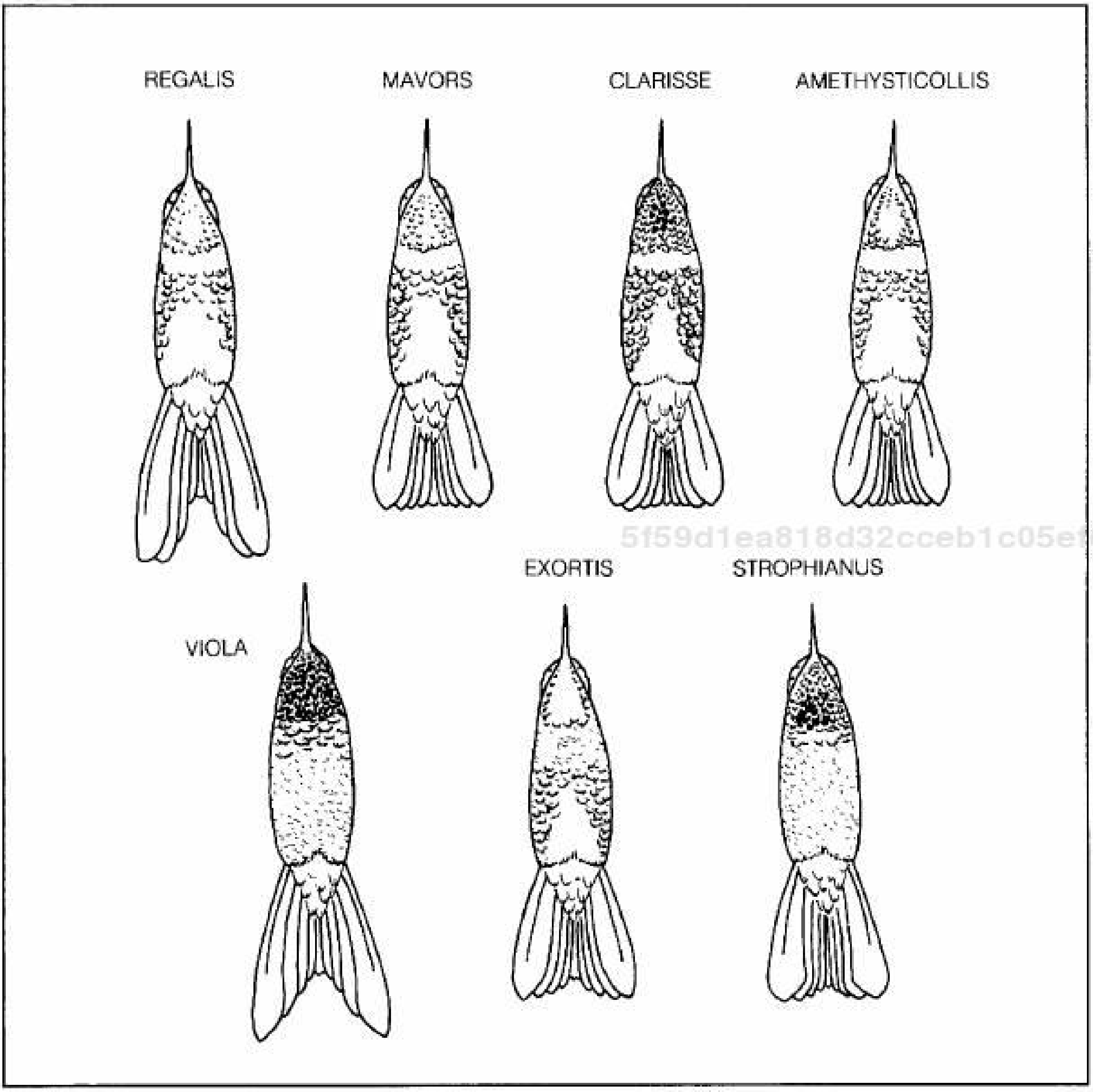

Fig. 3.1. Ventral patterns and tail forms of *Heliangelus* females, including *H. regalis*. *H. micrastur* female resembles *exortis*, and *H. spencei* resembles *amethysticollis*. Note similarity between *regalis* and *mavors*. Reprinted with permission from J.W. Fitzpatrick et al., "A New Species of Hummingbird from Peru," *Wilson Bulletin* 91 (1979): 180.

equally bright on both surfaces; combined with buffy underparts interrupted by a pale pectoral [breast] band" (p. 177).

Not only in the frontispiece, but throughout this paper, pictures join words in the clear display of likenesses and differences, strongly reinforcing the presence of a potential new species. In figure 3.1, for example, the seven drawings, considered in concert and without words, make equally emphatic both the family resemblances crucial for genus identity and the contrastive features essential for within-genus differentiation.

Identification: Taxonomical and Evolutionary Species

It is by means of statistical inference that this extraordinary detail is given taxonomical significance: by this means, Fitzpatrick and his co-workers definitely assert that they have a new species, a new living entity related to all such entities. Sixteen specimens have been collected:

	Adult	Subadult
Male	5	5
Female	5	1

Assuming a fair sample, the authors can describe the species by describing these specimens. Thus, when averages, ranges, and standard deviations are given (e.g., those for wing chord length), they are equally those of the specimens collected and of the species. Description, comparison and statistical inference having done their work, the species can be officially certified as new by naming: it is a hummingbird, the genus *Heliangelus*, the species *regalis*, a Royal Sunangel.

But taxonomical identification is not the final stage: taxonomic species must be redefined in evolutionary terms. A species first characterized without Darwinian assumptions is now characterized in terms of a theory concerning the origin of species: that all stem from the operation of natural selection on random variations, some of which are favored because they increase the rate of survival of those who possess them.

In taxonomic work, evolutionary theory serves at least two purposes: it explains problematic observations that arise in the process of identification, and it licenses prediction. In the process of identifying their new species of hummingbird, Fitzpatrick and his collaborators make the following problematic observation. Among the species of *Heliangelus* to which *regalis* seems most closely related, the monochromatic plumage of the male *regalis* is strikingly anomalous. To account for this irregularity, the investigators invoke the evolutionary concept of convergence (similarity in species resulting not from descent, but from environmental constraint):

> Thus, if *regalis* is indeed closest to this species as suggested by the appearance of the female, it has undergone a dramatic differentiation in which the male converged upon several more distant relatives. The elongated, narrow, metallic blue tail in both sexes of *regalis*, equally iridescent on both surfaces, is suggested only in *H. strophianus* (sexes similar) and *viola* (very large, dark green female lacks a breast band). (p. 181)

Presumably, this convergence is the direct or indirect result of as yet unidentified selection pressures.

But evolutionary theory does more than explain; it predicts. Prediction in evolutionary biology, of course, is not the same as in classical physics, where future events can often be forecast with startling quantitative accuracy. In evolutionary biology, although predictions need to be well defined and consistent with the evidence on which they are based, they can legitimately vary in precision of measurement:

> For example, population ecologists are generally satisfied to explain to one order of magnitude the increases and decreases in population size of the organisms studied, and for this purpose net fecundity and mortality are usually sufficient. Game and fish management, however, may require prediction of population changes to an accuracy of 10 to 20 percent, and for this purpose complete age-specific mortality and fecundity schedules are required. Finally, the human demographer needs to project human population sizes to better than 1 percent accuracy, and to do so needs fecundity and mortality figures by age, sex, socioeconomic class, education, geographical location, and so on (Lewontin 1984, 7–8).

Lewontin's principle is implicit in a report by Ricqlès and Bolt (1983). In this paper, these workers inquire into the origins and nature of "perhaps the most important captorhinid character," a feature whose evolutionary history they regard as a prerequisite to proper taxonomic use: jaw growth and tooth replacement, as exemplified in a prehistoric reptile, *Captorhinus aguti* (p. 7).[3] Their predictions, though far from precise, are nevertheless legitimate, since they are well defined and consistently applied: on the basis of large, clearly visible anatomical features, the scientists create a preliminary model. Later, they check the predictions of this model against effects of jaw growth and tooth replacement not visible to the naked eye. It makes sense that the dynamics of these processes should leave traces in surviving fossils, and that effects visible to the naked eye should be consistent with effects visible at between two and six magnifications.

Among the papers in evolutionary biology I have examined, falsifiability is mentioned almost as much as prediction. Falsifiability is prediction turned on its head; it asserts that a theory can be undermined by its repeated failure to predict. This notion has Sir Karl Popper (1965; 1968) as its best known exponent (see also Medawar 1984). For Popper, the possibility of falsifiability is at the root of all good science, all of which is open to being "*refuted by experience.*" In other words, it is the essence of science that we can refute theory with contravening facts:

> It is possible by means of purely deductive inferences (with the help of the *modus tollens* of classical logic) to argue from the truth of singular statements to the falsity of universal statements [in the texts examined, from "observations" to "hypotheses" or "models"]. Such an argument to the falsity of universal statements is the only strictly deductive kind of inference that proceeds, as it were, in the 'inductive direction'; that is, from singular to universal statements.

Popper recognizes that theories under empirical attack can always be saved "by introducing *ad hoc* an auxiliary hypothesis, or by changing *ad hoc* a definition." His solution is "the empirical method" with "its manner of exposing to falsification, in every conceivable way, the system to be tested" (Popper 1968, 41–42).

Two papers in my sample claim an openness to falsifiability. Writing about the spread of chipmunk populations, B. D. Patterson says of an hypothesis integral to his model that it is "apparently refuted by the presence of *E. canipes*, rather than *E. quadrivittatus* [species of chipmunk], in the Gallinas mountains"; according to this model, "small mountain ranges isolated from centers of endemism [areas where species originate] should have been colonized by southwardly advancing taxa," of which *E. quadrivittatus* is one (B. D. Patterson 1982, 393–94). In their paper, Ricqlès and Bolt assert that "evidence for successive occupation by several generations of teeth at the same site would falsify [their] model" of reptilian jaw growth and tooth replacement, a model that implies "that in the MR [multiple-rowed] area a given attachment site was occupied only once" (Ricqlès and Bolt 1983, 22). Since the geography of Arizona and New Mexico allows for the apparently anomalous presence of *E. canipes* in the Gallinas mountains, Patterson's hypothesis survives; since successive same-site tooth occupation is not in evidence, the conclusion of Ricqlès and Bolt still rests on firm ground.

The rational reconstruction of the concept of species by means of an analysis of typical papers in evolutionary taxonomy supports the view that science is an inductive progress to reliable knowledge, knowledge always open to, but nevertheless so far resistant to, falsification. The induction from which this knowledge proceeds has observations as its raw material. These observations are apparently free from the evolutionary theory that explains them. For example, the collection of sixteen similar birds by Fitzpatrick and his co-workers is a task possible to earlier, evolution-free centuries. It need have been motivated by nothing more than the hunch that the birds might form a new group. Of course, once the group has actually been segregated, it can be defined taxonomically; and, once it has been so defined, it can be redefined according to evolutionary theory. Evolutionary theory can then be used to explain and predict events and entities in the natural world and to retrodict these in the fossil record.

THE RHETORICAL RECONSTRUCTION OF SPECIES

Although Perelman and Olbrechts-Tyteca's *The New Rhetoric* does not contain any extended discussion of argument in the natural sciences—no examples from these sciences appear—there is no intent to exclude the natural sciences from the sphere of rhetoric. The authors believe that scientists address their very specialized audiences, audiences that are commonly very small indeed, as a "universal audience" in the sense "that everyone with the same training, qualifications, and information would reach the same conclusions" (Perelman and Olbrechts-Tyteca 1969, 34; compare Johnstone 1978, 91). By means of the universal audience, then, the natural sciences come within the sphere of rhetoric: the purposes of a rhetorical reconstruction would be served if we were to "characterize . . . the image [the evolutionary taxonomist] holds of the universal audience he is trying to win over to his view" (Perelman and Olbrechts-Tyteca 1969, 33). To characterize this image, we must find rhetorical equivalents for the stages of the rational reconstruction we have just presented: we must show how, *sub specie rhetoricae*, scientists create an ontology that persuades their fellows, an ontology in which plants and animals have been brought to life, raised to membership in a taxonomical group, and made to illustrate and generate evolutionary theory.

Creation: Potential Species

The first stage, the creation of the potential species, is best elucidated by the New Rhetorical concept of *presence*. Through presence, writers place "certain elements" in their discourses, those on which they "[wish] to center attention," in "the foreground of the [reader's] consciousness." Initially, therefore, presence is "a psychological phenomenon"; that on which the mind and senses dwell "is, by that very circumstance, overestimated" (Perelman and Olbrechts-Tyteca, 142, 116–17; compare Gadamer 103).

This reading of presence is consistent with Gestalt principles. According to these, sensations organize themselves into wholes, or *gestalten*, certain combinations of which seem automatically foregrounded; these combinations we see as having shape and substance, outlined against a shapeless and relatively insubstantial background. Gestalten are clearly manifested to sight and hearing, less clearly to the other senses. Attributively, they can refer not only to things experienced but also to collections of thoughts and ideas (Köhler 1947). On this reading, presence becomes a special case of perception.

Presence so conceived is easily subject to manipulation. In an ordinary way, we see an object that looks like a pencil; later, we grasp it and write with it, confirming our initial impression. But this simple conformity between our perceptions and the world, a conformity that encourages a naive realism, can be dramatically realigned: optical illusions and camouflage are well-known instances. Less dramatically, it is always possible to "adopt a special attitude with regard to the field so that some of its contents are emphasized while others are more or less suppressed" (Köhler 1947, 99). In visual fields, for example, manipulation makes both normal perspective and *trompe-l'oeil* possible.

In the description and depiction of the potential species of evolutionary taxonomy, presence is created by two devices: overdescription and multiple sensory perspectives. The first, overdescription, is the characterization of sense objects in detail far beyond a reader's ordinary expectations. In science, the rhetorical effect of presence routinely generated by overdescription has been analyzed by Steven Shapin (1984); he demonstrates that Robert Boyle's resort to overdescription in his experimental papers was a deliberate attempt to create "virtual witnessing," the use of circumstantial detail as a surrogate for the actual experience of confirmation through experimentation. In evolutionary taxonomy, overdescription serves an analogous purpose: on their own, words and pictures bring potential species to life.

In pursuit of their ontological goal, evolutionary taxonomists use, not only overdescription, but also multiple sensory perspectives. Fitzpatrick and his co-workers, for example, are no purists, looking for essential differences; all descriptive features are mustered that work to increase and individuate presence. This multiplicity of perspectives combined with the extraordinary detail with which each is described creates presence so effectively because it mimics the methods by which we confirm our everyday impressions of the reality of ordinary objects. We supplement visual data with more visual data from different angles and distances; the data of one sense with the data of another; qualitative sense data with mensuration. Thus, the presence of a species increases as the paper progresses; the "whole field of consciousness" becomes filled with this creature "so as to isolate it, as it were, from the [reader's] overall mentality" (Perelman and Olbrechts-Tyteca 1969, 118).

In scientific, as distinct from literary, prose the resources of language from which presence is created are decidedly limited. Because scientific prose is designed to create the impression that its language refers unproblematically to a real world existing independently of any perceiving subject, it generally excludes the subjective dimension of description, the use of emotion-charged words or irony. For the same reason, scientific prose generally excludes any device that shifts the reader's attention from the world language creates to language itself as a resource for creating worlds. These restrictions account for the unavailability of many of the techniques of presence discussed in *The New Rhetoric:* onomatopoeia is just one example of such an exclusion.

But language so constrained is hardly free from rhetorical effect; as Barthes cogently observes: "the zero degree . . . *is a significant absence* . . . the absence of rhetorical signifiers constitutes in its turn a stylistic signifier"; "denotation is . . . the *last* of the connotations . . . the superior myth by which the text pretends to return . . . to the nature of language, to language as nature" (Barthes 1967, 77–78; 1974, 9). Since the creation of presence in science is limited to those devices by which language may be said to refer unproblematically to a real world, it seems fair to give that sort of presence a name: to call it *referential presence*.

Creation: Taxonomical Species

We can account for referential presence *sub specie rhetoricae;* but can we account for its taxonomic transformation? We can, if we can show how statistical inference, naming, and artistic rendering create the persuasive structures that transform referential presence into taxonomical species.

In evolutionary taxonomy, statistical inference is designed to convince readers that speciation partakes of the certainty generally attributed to a branch of mathematics. There is no question of the importance to evolutionary taxonomy of this frequently used, routinely foregrounded set of techniques. But it is far from clear that statistical inference is the actual basis for the establishment of new taxa. The validity of such inferences depends on the efficacy of sampling procedures: generalizations from a sample can be made only on the basis of random selection from a defined population. In practice, limitations of time and funding force reasonable compromises. But Fitzpatrick and his co-workers define *Heliangelus* *regalis* by means of sixteen birds! How can they assert that these are representative? This practice of unsystematic sampling, which seems the taxonomic norm (Sokal and Crovello 1984, 544, 558), provides a clue to the rhetorical nature of the taxonomic transformation.

In truth, taxonomic speciation depends less on statistical inference than on a fundamental presupposition about the existence of species in the order of nature. These species must necessarily be imagined as "discrete sets with appreciable distances between them." Taxonomic speciation absolutely "depends upon the possibility of grouping data in clusters with empty space between them" (Kuhn 1977, 312 and note). This continuity within, and discontinuity between, species is perfectly in accord with Gestalt psychology: between gestalten there is "a 'dead' interval which corresponds . . . to the mere extension or ground outside a visual shape" (Köhler 1947, 111). But this presupposition is not in accord with the conviction that species are established solely on the basis of statistically inferred regularities.[4]

A striking instance of the operation of the presupposition concerning the essential nature of species, culled from the papers I examined, concerns *Trachelyichthys decaradiatus*—a new species of a new genus of catfish identified and described on the basis of a single specimen! (Greenfield and Glodek 1977) While statistical inference is invoked, another, antithetical procedure is actually used. Greenfield and Glodek simply assume synecdoche: a single creature automatically evokes the order of nature as a taxonomic network. Forewarned by such imaginative leaps, one may question the legitimacy with which the precision demonstrated for specimens is routinely transferred to the species.

Artistic rendering also confers taxonomic identity. In Fitzpatrick, Willard, and Terborgh on hummingbirds, the full-color portrait of *regalis* in situ may strike the naive viewer as a candid glimpse. But this picture is posed: it presents these creatures in a manner designed to display, not just any characters, but only those that best distinguish them from their fellows. In the case of the line drawing reproduced in Figure 3.1, the purposes of taxonomic identification are served by another form of visual rhetoric: tendentious simplification.

Finally, naming confers taxonomic identity. As Victor Turner has demonstrated for the Ndembu (Turner 1981, 85–86, 180), and David Hull has shown for taxonomy (Hull 1984, 638), naming is a carefully guarded cultural resource whose purpose is to bestow identity: taxonomic names bestow taxonomic identity. Although taxonomic naming persuades us to credit the scientist with discovery, on the contrary,

> wherever language and men exist, there is not only a freedom from the pressure of the world, but this freedom from the habitat is also freedom in relation to the names that we give things, as stated in the profound account in Genesis, according to which Adam received from God the authority to name creatures. (Gadamer 1975, 402)

The verbal portrait of the hummingbird consists of scattered islands of detail. Naming alone resolves the paradox by means of which this scattered detail becomes a new creature, but one for whom a taxonomic space already exists (Barthes 1974, 60–63, 94–95, 209–10).

Creation: Evolutionary Species

The evolutionary species is part of at least two networks of meaning, two versions of evolutionary theory. In the first, or strong, version, the species is an integral part of a formulation that aspires to make quantitatively precise forecasts of events in space-time, events directly inferred from mathematically expressed physical laws. In the second, or weak, version, this same concept is an integral part of a formulation that brings together a large and otherwise disparate number of phenomena in the natural world under a single conceptual umbrella.

Darwin himself is the source of both versions of evolutionary theory. In *The Origin*, taking his cue from the explanatory model of classical physics, he says in support of the strong version:

> Throw up a handful of feathers, and all must fall to the ground according to definite laws; but how simple is the problem where each shall fall compared to that of the action and reaction of the innumerable plants and animals which have determined, in the course of centuries, the proportional numbers and kinds of trees now growing on the old Indian ruins! (1962, 86)

In his letters, writing in the same vein, he compares his theory with gravitation and with the wave theory of light (Darwin 1972, 1:150; 1959, 2:80, 83–84).

At other times, however, Darwin seems to have for evolutionary theory explanatory goals that are far less rigorous. In his letters, he also says: "an hypothesis is *developed* into a theory solely by explaining an ample lot of facts"; it "[connects these facts] under an intelligible point of view." In a letter to Lyell, though he ventures a weak prediction concerning the fossil record, Darwin specifically abjures strong prediction, which he calls "prophecy" (1959, 2:80, 210, 9–10). Indeed, as a whole, *The Origin of Species* exemplifies the weak version of evolutionary theory. But to adopt this version is also to abjure falsifiability as a criterion for the adequacy of theoretical formulations.

Are contemporary taxonomists strong or weak theorists? In contemporary taxonomy, as in Darwin, the weak version of evolutionary theory is predominant. In their work on a new species of hummingbird, Fitzpatrick and his collaborators are typical in their use of convergence to explain an anomalous character; in so doing, they are using evolutionary theory to connect "an ample lot of facts . . . under an intelligible point of view." In the papers examined, however, Ricqlès and Bolt, and B. D. Patterson clearly underwrite falsifiability as the chief criterion for the theoretical adequacy of their formulations. In so doing, they seem to accept the evolutionary species as part of a strong theory. But appearances are deceptive. In his analysis of falsifiability, Popper insists that the element of risk be maximized: "an attempt to solve an interesting problem by a bold conjecture, *even (and especially) if it soon turns out to be false*" (1965, 231). Although the statements of Ricqlès and Bolt look Popperian at first glance, careful scrutiny reveals the hyperbolic nature of their claims. In these papers, the theories of their authors are not seriously at risk; a fortiori evolutionary theory is never seriously at risk.

In truth, Ricqlès, Bolt, and Patterson take advantage of the impressiveness that an openness to falsification confers without taking the risks that such openness should entail. What Ricqlès and Bolt identify as falsifiable is not even central to their argument; it is only their preliminary model, a model which is no more than an artifact of their method. They elected to build this model from large, clearly visible anatomical features, and to correct it by means of anatomical features not visible to the naked eye. As they readily admit, they could have built their final model directly and entirely from submacroscopic features. If they had, potential falsifiability would have disappeared, since "[submacroscopic] evidence for successive occupation by several generations of teeth at the same site" would have been clearly absent from the beginning (Ricqlès and Bolt 1983, 22).

Patterson is no truer to Popper than are Ricqlès and Bolt. Because his model really is endangered by some awkward evidence, it is technically falsifiable. But, to save it, Patterson resorts to an ad hoc hypothesis, expressly forbidden by Sir Karl: "An elevated corridor . . . extends from the White Mountains north to the Gallinas Range. Chipmunks *evidently* [my italics] were able to move through this corridor to colonize the Gallinas

Range before it could be reached by *E. quadrivittatus*" (B. D. Patterson 1982, 395). One can only conclude that falsifiability is invoked in both these instances to give a mistaken impression of the strength of particular taxonomical claims.[5]

Despite differences, however, the weak version of evolutionary theory is importantly similar to the strong. In both, natural selection creates the phylogenies, the lines of descent, we observe in nature and in the fossil record;[6] in both also, evolution is a process theory and a theory of descent. These shared theoretical resources allow taxonomists to use either version as a completely unquestioned, utterly reliable source of intellectual underpinning, an ideational account on which they can reliably draw to explain and extend their observations. Since evolutionary theory founds evolutionary biology, by definition it has axiomatic status: in every paper, its appropriateness in species identification can be taken as settled.

In fact, it is far from settled. When Sokal and Crovello assert that taxonomy can proceed very well without the evolutionary species, "a theoretical ideal to which existing situations are forced to fit as closely as possible" (1984, 560), they are not alone, nor is their alternate approach—phenetics—without adherents. Nor is phenetics the only alternative. An advocate of "transformed cladistics" believes "that much of today's explanation of nature, in terms of neo-Darwinism or the synthetic theory, may be empty rhetoric" (C. Patterson 1982, 119).

At the heart of this problem is a paradox inherent in any taxonomic affirmation of evolutionary theory. If the theory is right, species cannot be natural kinds, entities with atemporal identities, like genes or electrons: full knowledge of intermediary varieties would demonstrate their wholly historical nature. Concerning Ernst Mayr's classic, *Systematics and the Origin of Species*, Niles Eldredge says: "His treatment of the entire problem implies a recognition that if one goes too far in embracing the principles of natural selection and adaptation as the be-all of evolution, the systematist [taxonomist] is left with nothing to explain" (1982, xix). In other words, any version of evolutionary theory eventually leads to the disappearance of the species as a legitimate natural kind. The evolutionary species is a rhetorical construct, an oxymoron created only by avoiding the full implications of the theory on which its existence apparently depends.

THE RHETORIC OF SCIENCE AS A DISCIPLINE

In their recent article on a contemporary controversy in evolutionary biology, John Lyne and Henry F. Howe (1986) demonstrate the possibility of doing first-rate rhetoric of science in the absence of explicit theory. Such a move—toward rhetorical criticism and away from theory—is understandable: one remembers how the call for a rhetoric of science decades ago ran aground when a surge of theory was unaccompanied by the outpouring of criticism that would have given that theory substance. But if the rhetoric of science is to establish itself permanently as a branch of inquiry, theory and practice must work together. By itself, rhetorical criticism can produce cogent and persuasive analyses of individual texts only by means of tacit theoretical assumptions; coupled with an adequate, coherent, and explicit theory, such criticism can establish the additional claim that the rhetoric of science is a discipline in its own right.

Joseph Gusfield makes a strong claim for the adjunct status of rhetorical analysis: "The rhetorical component *seems* to be unavoidable if the work is to have a theoretical . . . relevance. Thus an analysis of a scientific work *should* . . . include its rhetorical as well as its empirical component" (1976, 31; his italics). Gusfield's claim depends on a distinction that privileges science at the expense of rhetoric. This view has a clear and venerable source: Aristotle (trans. Ross 1971) asserts that rhetoric is not a field because it lacks a proper subject matter; furthermore, "in so far as anyone tries to construct either dialectic or rhetoric not as a knack but as a science, he will unconsciously destroy their nature, by passing over, in his attempt to reconstruct them, into sciences of definite subject-matters, and not of mere arguments" (1359b12–16). In issuing this warning, Aristotle shares with Gusfield a view of the epistemological superiority of the nomological over the hermeneutic disciplines.

When this mistaken view is set aside, the way is open to acknowledge the disciplinary status of rhetoric. As its subject matter, rhetoric has the persuasive structures of all fields, itself included. As the study of such structures in the sciences, the rhetoric of science presupposes that all components of disciplinary discourse are within its explanatory compass: there is no empirical or theoretical core, no essential science that reveals itself all the more clearly after the rhetorically analyzed components have been set aside. As a discipline, rhetoric may be expected to behave differently from classical physics: one does not expect laws of rhetoric or rhetorical predictions. But the disciplinary claim is fundamental; without disciplinary status, the rhetoric of science is not a field, but a bundle of techniques, an adjunct to fields.

When we ignore Aristotelian biases, we can easily set rhetoric beside such hermeneutical disciplines as history, Bible commentary, and literary criticism; indeed, its disciplinary status is far more ancient: the case can be made that it was the first organized body of hermeneutic study, the *ur-Geisteswissenschaft*. Throughout its long career, rhetoric has been overshadowed first by philosophy, then by science, both of which limited its scope, and trivialized its usefulness. But such limits and such trivialization are not inherent in the nature of rhetorical theory; rather, they are the result of a continuing need in Western civilization to open up a space for the chimera of certain knowledge.

AN EPISTEMOLOGICAL PROBLEM

The disciplinary claim for rhetoric of science has as its source a fundamental federalism about the domains of knowledge. In accord with this federalism, rhetoric is one discipline among many in a joint enterprise, a confederation of equally sovereign intellectual states. A central authority is deliberately absent; claims to such authority—the traditional claims of philosophy and theology—are wholly without merit. No discipline is, or can be, privileged over another. This paper exemplifies the absence of such privilege: it reconstructs evolutionary taxonomy in two equipollent ways.

According to its rational reconstruction, evolutionary taxonomy is a discipline at whose center lies the evolutionary species; in fact, belief in the reality of such species is the sine qua non of being an evolutionary taxonomist. This rational reconstruction of science is both justificatory of, and coextensive with, science itself. For rhetoricians of science, on the other hand, the reality of the evolutionary species is essentially textual. Scientific texts are constituted through a symbolic interaction among scientists, an interaction constrained only by the recalcitrance of nature and by the cultural history embedded in language and lives. Rhetoricians insist that through this interaction scientists "*establish the real. The real* is as much a hypothetical construct as is the universal audience" (Karon 1976, 103).

Rational and rhetorical reconstruction differ in their fundamental motivation. Rational reconstruction is clearly subsumed under "the cognitive interest in technical control over objectified processes" (Habermas 1971, 309; see also 212). Rhetorical reconstruction, on the other hand, is a manifestation of the practical interest "oriented toward mutual understanding in the conduct of life." This interest is "directed toward the transcendental structure of various actual forms of life, within each of which reality is interpreted according to a specific grammar of world-apprehension and of action." The structures of persuasion of evolutionary taxonomy are part of such a grammar (Habermas 1971, 311, 195).

Both reconstructions of evolutionary taxonomy are equally legitimate; each, though incomplete in the larger view, is complete in itself. But an autonomy so radical creates an epistemological problem. Each interest, the technical and the practical, has its appropriate epistemology, its bundles of pertinent analytical techniques. By way of contrast, the theoretical vantage of this paper lacks legitimation. By what right has evolutionary taxonomy been reconstructed, its reconstructions compared, and their ontological equipollence inferred?

AN EPISTEMOLOGICAL SOLUTION

Traditionally, transdisciplinary objectivity depended on our ability to "rise above" the narrow views of the "petty" kingdoms of disciplinary knowledge; to move outside their "confining" boundaries; to transcend their "parochial" concerns. Such objectivity licensed the class of statements at this paper's theoretical core. But our analysis of evolutionary taxonomy seems to confirm Wittgenstein's well-established view that objectivity consists in living by the laws of one intellectual state or another; in Wittgenstein's terminology, one form of life or another. Outside of these, there is apparently nothing, no real world we can use for our criterion.

So strict an interpretation of Wittgenstein has a certain plausibility. In particular sciences, there is the common experience that central concepts increase in ontological status, in objectivity, wholly within a single domain of interest, the technical. Did not Einstein's papers on Brownian movement put to rest any doubt about atomic reality? Did not Watson and Crick's discovery of the structure of DNA finally give the functional description of the heredity process a firm physical sense? But additional reference to the history of science reveals the incompleteness of this view. Galileo's battle against geocentricity, Young's struggle against the particle theory of light, Darwin's war against the argument from design: in each of these instances, a central concept, a scientific "fact"—geocentricity, particle theory, design—was revealed as a presupposition rooted, not only in the technical, but also in the practical interest, in tradition and in current, widespread use.

Indeed, in Habermas' view, it is in the practical interest that the true source of scientific objectivity lies. To agree with Habermas on this point is to say that science is a form of social action; that it progresses only in so far as it enhances human survival. Why does it often seem otherwise, especially to scientists?

> Because science must secure the objectivity of its statements against the pressure and seduction of particular interests, it deludes itself about the fundamental interests to which it owes not only its impetus but *the conditions of possible objectivity* themselves (Habermas 1971, 311).

In this passage, Habermas explains the narrower sense of objectivity we so often meet in the sciences, and motivates a broader sense, one that encompasses both the technical and practical interest.

A strict interpretation of Wittgenstein, then, leads to a misunderstanding of science as an objective and rational pursuit. The rational and the rhetorical are far from opposites. Their alleged opposition, so deeply a part of our intellectual heritage, is nothing more than a rhetorical fiat in need of challenge. To undermine the Sophistic tenet that knowledge was rhetorically constituted, Plato and Aristotle drew a firm line where none had existed. Above *doxa*, a knowledge no more privileged than its knowers, they raised *episteme*, true knowledge: the complete and accurate depiction of an independent reality.

Without the long endorsement of this Platonic dream, an endorsement whose influence still strongly lingers in Western thought, would we not see similarity where we now see disparity? Where would definition by family resemblance be without the *topoi* of genus and difference? Need classification be independent of the *topos* of comparison? Is there an *essential* difference between scientific and rhetorical description? Between scientific and rhetorical definition? Is not the application of evolutionary theory a systematic use of the *topos* of antecedent and consequence? Is not the conflict between the rational and the rhetorical a civil war?

To negotiate a truce in this conflict, we need to draw on a sense of objectivity and rationality broader than that of any discipline. This broader sense has its source in the third and most important of Habermas's three human interests, the emancipatory, an interest one of whose purposes is to "[destroy] the illusion of objectivism [cultivated by the sciences] through demonstrating what it conceals: the connection of knowledge and interest" (Habermas 1971, 316–17). The vehicle of this emancipatory interest is critique, a discourse on discourse, dialectic in mode, ironic in manner. It is by means of critique that we reconstruct evolutionary taxonomy in two very different ways, and compare these reconstructions.

It is with critique that I would identify a broader, transdisciplinary sense of rhetoric, a sense different from that exemplified earlier by the application of the conceptual machinery of Aristotle's *Rhetoric* to the texts of evolutionary taxonomy. In this sense, rhetoric is not a discipline, but a perspective whose essential character is reflexive and ironic.[7]

In an apt political metaphor, Kenneth Burke captures this perspectival sense of rhetoric:

> Insofar as terms are thus encouraged to participate in an orderly parliamentary development, the dialectic of this participation produces (in the observer who considers the whole from the standpoint of the participation of all the terms rather than from the standpoint of any one participant) a "resultant certainty" of a different quality, necessarily ironic, since it requires that all the sub-certainties be considered as neither true nor false, but *contributory* (Burke 1962, 513).

This seems exactly right. From the point of view of rhetoric as critique, the rationality of science consists in the continuing dialectic among its legitimate reconstructions, each the surrogate for the informed assent of an interpretive community; analogously, the objectivity of science is constituted by some configuration of these reconstructions. Such configurations must be viewed ironically; only if irony is presupposed do they avoid the charge of inconsistency.

CONCLUSION

Rhetoric is both a discipline and a perspective from which disciplines can be viewed. As a discipline, it has a hermeneutic task and generates knowledge; as a perspective, it has a critical, emancipatory task and generates new points of view. The central goal of this paper is not hermeneutic, but critical and emancipatory: a new perspective is elaborated, illustrated, refined. If the job of critique has been successfully accomplished, the reader will have been persuaded of the untenability of a sharp distinction between rhetoric and rationality. She will have been convinced that such a distinction entails an unduly narrow view of both rhetoric and rationality, and a gratuitous denigration of human-centered knowledge. Finally, she will have been persuaded that evolutionary taxonomy is a species of such knowledge.

NOTES

1. Although scientific characterization of the species is a subject of serious controversy (Hull, 1981a, 1981b, 1983a, 1983b), I think these views are defensible.

2. This orthodox view is not held by radical pheneticists (Hull 1981a).

3. Systematists do not only use characters; they also "[inquire] into the origin and nature of the units with which [they work]" (Mayr 1982, 9).

4. According to Hull (1983a), it is a general practice in much of biology to deal with species as natural kinds: "though comparative anatomists clearly acknowledge that species evolve, they insist that they can go about their business as if they did not" (p. 76).

5. I dwell on Popper because scientists do; I am far from holding up his ideas as an accurate way to reconstruct good scientific practice. Whatever the status of evolutionary theory in relation to taxonomy, taxonomists have successfully practiced for centuries a science that is simply not about bold conjectures and crucial observations. Indeed, it is even questionable whether physics, Popper's obvious model, is such a science. Let us glance at one famous instance: the bending of light in a gravitational field, designed as a crucial test of general relativity. In this instance, the data physicists found so convincing exhibited a scatter far wider than any ordinary canons of proof would allow (Bernstein 1985, 141–46; compare Franklin 1986, 226–43). In the end, as Bernstein makes clear, physicists were persuaded as much by the elegance of the theory as by its alleged resistance to falsification, a resistance that, under different circumstances, could easily have been given the opposite interpretation. What is true in physics is true a fortiori in evolutionary taxonomy: "The reason for taxonomists paying so much attention to the works of Popper is that they think that they can use his Principle of Falsifiability to show that *their* classifications are truly scientific, while those of their opponents are not" (Hull 1981a, 142).

6. It is also possible to hold with the cladists that evolution is only a theory of descent. This narrower version of evolution does not seem pertinent to papers in my sample.

7. These two meanings of rhetoric correspond to Husserl's distinction between two sorts of reflection: the natural and the radical (Carr 1974, 16–27).

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